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XV.—THE PROJECTION OF THE RETINAL IMAGE.

BY W. B. PILLSBURY, PH. D.

It would seem that the extended discussion of the problem of the "Inversion of the Retinal Image," carried on in the *Psychological Review* for the current year, would leave nothing to be said on the above question. One bit of experimental evidence has been overlooked, however, which may be of value in settling disputed points in both of the intimately connected problems. As the discussion stands at present, Professor Stratton has shown that, when the field of vision is inverted by means of lenses, it is possible in a comparatively brief time so to adjust the tactual space to the new visual space that no errors in movement result, and there is no conscious disparity between the two spaces. Professor Hyslop rejoins that this is 'off the point,' because the experiments only cover the question of adapting movements to visual space, which is very different from the question of reference within the visual space itself; and, that, first of all, we must prove that inverted vision is an anomaly. On the constructive side, Professor Hyslop maintains that it is the general law, and therefore natural, that all impressions on the retina should be referred backward along a line perpendicular to that surface, just as in touch we 'feel' with the end of a stick in a line perpendicular to the surface of the skin.

It is with this law that our own observations primarily deal. Professor Gage of Cornell University called our attention to the fact that in using the Abbé *camera lucida*, in drawing from the microscope, the double image was invariably referred to the drawing board, not to the stage of the microscope. In view of the fact that the latter is in the line perpendicular to the retina, or is in the projection of the univalent ray, we seem here to be dealing with an anomaly of vision, and one worthy of careful investigation. Owing to the construction of the instrument the ray from the paper on the

drawing-board is reflected twice before it reaches the eye: once from a mirror above the board, and once from a silvered prism above the ocular of the microscope. The ray from the slide under the objective, on the other hand, passes in a straight line through an opening in the silvering of the prism; so that the rays from drawing-board and slide enter the eye along the same line, and occupy a common position upon the retina.

Here we have a practical test of the comparative truth of the projection or nativistic and the empirical theories. The eye is offered a choice between two possible lines of reference, the one of which it must take if the projection theory be true, and the other of which it may take if the empirical view be correct. It would not be necessary that both images should be referred to the drawing-board, if we assume the empirical standpoint. Even on this theory we are opposing the experience of the individual and the race, that objects can be touched somewhere in the line perpendicular to the retina at the point of stimulation, to the present knowledge that we can touch the paper by the side of the microscope. This latter knowledge, furthermore, is not unambiguous, because we have had fully as many tactual indications that the preparation is under the objective of the microscope as that the paper is on the drawing-board to one side. Particularly is this the case if, as is true most frequently, the student is accustomed to the use of the microscope and unfamiliar with the *camera lucida*. Every circumstance, then, favors the expectation that the double image will be referred to the stage of the microscope, except the fact that we expect to draw the image upon the paper, and are intent upon seeing it against the white back-ground under the point of our pencil.

In spite of the predominance of theoretical considerations in favor of the direct line, the fact is that in every instance the combined image is referred to the drawing-board. Professor Gage assures me that he has never known an exception to this rule in all his experience with classes in microscopy in Cornell University, which have been composed in the aggregate of many hundred students. We made a number of experiments, when at an early stage in the use of the *camera lucida*, with the mirror in different positions; and found that always, when an object of known position was recognized, it was referred to its true place; before it was recognized it was referred to the stage of the microscope. Once, by accident, the black metal surface of the back of the mirror was turned toward the tube, in a position to reflect the ceiling of the room; and even this dark, indistinct image was assigned to its proper place as soon as it was recognized. This observation was made after only two hours' work with the *camera*

lucida two days before. It must be noted that in all these cases the objects in the mirror were being constantly changed, while the field of the microscope was not varied. Interest and attention were, therefore, directed almost entirely to the kaleidoscopic panorama of the former, just as in the usual case they were directed to the image that was to be drawn. It would seem, then, that this phenomenon is very closely related to retinal rivalry; and that the images are referred to the place we are most interested in at the moment. This means that our knowledge that an object occupies a certain position is practically as strong in producing reference to a point out of the normal line of projection as to a point in that normal line; or that *the direction of the ray which gives rise to a retinal stimulation is comparatively unimportant in determining the place in space to which we shall attribute the origin of the stimulation.*

These observations alone would be decisive against the projection theory in any of its forms. The rashest nativist would not care to assume that the retina reacts differently to light that has been reflected and to light that has not been reflected, or that we can know how many and of what magnitude are the angles that the path of the ether vibrations has described in its course to the eye. And this hypothesis is also excluded in our experiments by the fact that the bent ray is preferred to the direct ray.

We have, then, met Professor Hyslop's¹ first and second conditions: that we should prove that inverted vision or projection outward in a line perpendicular to the retina is not natural, or is not the only natural method of reference; and that the proof should be in terms of vision alone. We may now consider his objection to the empirical theory that it assumes a knowledge of the visual impression, both before and after reference outward. This is, of course, a mere figure of speech, which is employed for convenience by both sides. It is no more to be taken literally than Professor Hyslop would care to have a critic work out his conception of nature and natural law from the sentence:² "Now accepting Mr. Spencer's conclusion that the sense of touch was the original germ out of which all the senses were developed, among them sight, we can readily see that nature had only to give the retina a curved form, circular, elliptical or parabolic, in order to adjust the law of 'eccentric projection' to the modified conditions of vision involving refraction of light and inverted images." No one of normal development can

¹ *Psy. Rev.*, IV, 2, p. 151.

² *Psy. Rev.*, IV, 2, p. 160.

remember a time when reference to a point in space did not take place, or a time when the images were referred to the retina ; because reference to the retina involves knowledge or experience in space just as much as reference to a point remote from the body. The facts are apparent to all ; but it is very difficult to represent them or discuss them without falling into figures of speech. Professor Hyslop himself does not escape the danger.

We have, then, no course left but to accept the empirical theory ; for the 'eye-movement theory' has never been held by anyone to be a sufficient explanation in itself, and has also been disproven by both Professor Stratton and Professor Hyslop.

The empirical theory seems the more applicable to direction, because there is no function of the eye that in any way corresponds to this attribute. The reaction of the retina and of the ciliary muscle is the same, no matter from what direction the exciting stimulus may come. We are, then, compelled to look to some sense besides vision to supply the deficiency ; and touch, the other spatial sense, seems the most natural recourse.

The question from this point on is the question of the genesis of space perception ; and within the limits of a paper of this kind it is only possible to remark that all evidence points to the conclusion that space is a composite, and that in the normal consciousness more than one sense must always be at work in its development. We therefore conclude with Professor Stratton that the phenomena of projection can only be explained empirically by calling to our aid the sense of touch.

POSTSCRIPT.—(1) It should be stated, in connection with Dr. Pillsbury's explanation of the lateral projection of the image when Abbé's *camera lucida* is placed over the ocular of the microscope, that the Cornell students pass to the use of the Abbé instrument by way of the Wollaston *camera lucida*. Hence they may come to the former with a predisposition which is too strong to be broken by their new-gained knowledge of its construction and dioptrical properties.

(2) After reading Dr. Pillsbury's paper, it occurred to Professor Gage that it would be well to repeat the observations on the Abbé camera. He found that—although in the whole course of his previous experience he had localised the preparation on the drawing-board—he was now able at will to see it either there or in the microscope. Professor Gage came to the Abbé by way of the Wollaston instrument. Again : he has found two persons who are at present unable to localise the preparation elsewhere than in the microscope.

The one is wholly unpracticed in the use of the microscope ; the other is a practiced microscopist, who did not, however, use the Wollaston until after he had used the Abbé instrument. My own experience (by no means extended) is as follows : I have never used the Wollaston camera ; but I invariably localised the preparation on the drawing-board during a short time in which I was working with the Abbé camera. My first observations were taken without, the later ones with, theoretical knowledge of the instrument. Now, on making observations with Professor Gage, I find that I am able, as he is, to see the object with either location.

It seems, both to Professor Gage and to myself, that this criss-cross of evidence and experience tells strongly in favor of Dr. Pillsbury's main contention : that localisation demands an empiristic explanation. E. B. T.

NOTE TO STUDIES IV AND VII.

To their work upon the quantitative determination of the dotted-line and point-distance illusion (this JOURNAL, Vol. VI) Messrs. Knox and Watanabe appended certain theoretical remarks. Two of these run as follows :

(1) Since our judgment of vertical distances is in general less accurate than our judgment of horizontal, we should expect to find a higher value of Δ in the former case than in the latter.

(2) Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of overestimation of the upper part of the field of vision. We should, therefore, expect to find the *m. v.* of our vertical Δ 's greater than that of our horizontal.

These two remarks were very sharply criticised by Professor Heymans in the *Zeitsch. f. Psych.*, Vol. X, p. 465. As the criticism seemed to depend upon a misunderstanding (for which the brevity of the remarks themselves might be largely responsible), I published a full explanation in the *Zeitsch.*, Vol. XII, pp. 395, 396. By "less accurate" in (1) we had meant to indicate that the difference-limen is greater in judgments of verticals. The reasoning underlying (2) is somewhat complicated, and need not be repeated here.

Professor Heymans has replied to the first part of this explanation in the *Zeitsch.*, Vol. XIII, p. 474. In his reply he has entirely overlooked the fact that the upper and lower difference-limena must be of different magnitude if Weber's law holds, *i. e.*, unless the absolute sensible discrimination is constant. The estimation-difference Δ follows the same law as the sensible discrimination. I still think, therefore, that our first remark is justified. On the second point Professor Heymans offers no further criticism. E. B. T.